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Cyclones and Anti-Cyclones" by Zh D ZubyanPLACE ACQUIRED
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1. "The Origin of Cyclones and Anti-Cyclones" by Zh D Zubyan,* published in the USSR in 1949, represents an attempt to acquaint Soviet field meteorologists with the latest developments in the year 1949 concerning the origin and development of cyclones and anti-cyclones. Therefore it is concerned with the problem of atmospheric pressure changes, the movement and development of weather systems and consequently with the weather itself. The basic theory which is reviewed in this paper is called the advective-dynamic theory. It is based on a method developed by I A Kibel in 1940 which has undergone further development at the hands of several other Russians mentioned in the article. The most recent impetus given to Kibel's method seems to have occurred about 1947. Kibel's method is one for computing quantitatively the atmospheric pressure changes. This is done in the method by means of two different approximations. He calls them the first and second approximations. The word advective in the term "advective dynamic" refers to this first approximation, and the word dynamic refers to the second approximation. The total pressure change equals the sum of the two. The approximations are difficult to explain in words. Perhaps the best thing to say is that the advective part of it, or the first approximation, is based on the first law of thermodynamics, primarily a thermal relationship; while the second, or dynamic approximation, is based on equations of motion. In both cases there are very limiting assumptions introduced into these equations.
2. Now it is obvious that the soundness of this advective-dynamic theory will depend entirely on the soundness of Kibel's method, and I think I could say immediately that in my estimation its theoretical basis is very shaky. Kibel does show a knowledge of the basic meteorological equations and considerable ability to manipulate them. However, in making the assumptions and approximations that must inevitably be made in order to render meteorological equations tractable, he arrives at expressions that correspond only slightly with physical reality as we know it in the US through our synoptic studies of the past decade. This cannot be attributed to inadequate observed data. As far as I can gather from reading the article and

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also from other things I have read in the popular press and from my knowledge of the weather data available today, it appears that the Soviet observing network is comparable to our own. In other words Kibel has available to him, especially through international cooperation, the same basic weather data that we have. There is, however, more to this than meets the eye, as I will illustrate in a minute. A lot of this is no reflection on Kibel or his ability. I might just say here that qualitatively Kibel's first approximation says that warm air advection and falling pressure go together. This is something we know to be roughly true from our own empirical studies here in the US. But what I feel is that it is little more than an accident, or perhaps that it is just juggling with this approximation that yields quantitative results of proper magnitude.

3. A summary of Kibel's method written by a man named Izvenkov appears in the November 1946 issue of the Bulletin of the American Meteorological Society. However, it is only a summary. It does not carry through the theoretical development completely. There are large gaps in the mathematics which are next to impossible to fill in. And so in criticizing the theoretical basis of it, it should be taken into account that the summary in the Bulletin is incomplete and we have to be a little bit cautious in our criticism. The following article in the same issue is by Professor Bernhard Haurwitz, one of our leading American meteorologists, who writes on the results of a test of Kibel's method carried on at the Massachusetts Institute of Technology in the year 1944. This was no attempt to debunk the method. The test was entered into enthusiastically. Nothing was known about the method at the time except that the Soviets made great claims for it. He hoped at that time actually to gather something of worth from it. Judging from Kibel's theoretical development I would have been amazed to find anything of value coming from the applications, but perhaps I have an advantage here of having read both articles more or less simultaneously at a later date. At any rate the basic conclusion of Dr Haurwitz was this: "The attempts made at MIT to apply Kibel's method to the construction of prognostic pressure charts were thus not successful, contrary to the experience of the Russian meteorologists". Yet Zubyan in 1949 is still claiming success, and although he does hint that there is some disbelief among the Russians themselves, he still defends it staunchly.
4. Now let's get down to this question of whether the shortcomings of this method constitute any real reflection on Kibel. I think the important thing to remember is that it was written in 1940; and it was actually better than much of our own work of that period. We were making similar attacks on the same problem and usually not quantitatively. Kibel's method represented quite an advancement in the sense that he was daring enough actually to try to compute these things. He was of course not using a computer. He was computing instantaneous appearances in the atmosphere, and this method allowed to a certain extent for the extrapolation of this basic trend into the future. It was not similar to our own current attempts at numerical prediction in the US, but basically I see no reason why he couldn't have used the same iterative processes that we use. In other words, his methods allowed him to compute changes in pressure and temperature. That gave him a new field of pressure and temperature and on the basis of the formula which he developed he could keep on repeating this process. He could have done this in a fashion similar to what we do, but he didn't. He more or less just got the instantaneous trend of events and extrapolated them into the future. That whole phase of the work can be checked in Haurwitz's article, because Haurwitz in describing his own testing of the method used similar methods to those suggested by Kibel.
5. I get the impression from Zubyan's article that the Soviets must have had frustrating experiences with Kibel's method quantitatively and that they are at the point where they use it only qualitatively. What they have done is to develop a whole series of qualitative rules based on this formula. We dropped the line of research indicated in Kibel's method along towards the mid-1940's. I should qualify this by saying that there is still research in this country of lower caliber even now which is along basically similar lines; but I should emphasize again that the higher caliber research in this country has been along entirely different lines beginning in the middle and late 1940's. This advective-dynamic theory which is built around Kibel's equations is supposed to have been originated in 1947. It is hard to believe that the Soviets were still accepting Kibel's method as fact in 1949, but judging from Zubyan's article they seem to have been. At the time they told the man out in the field, the man of meager training, that it was one big development in Soviet meteorology in the 1940's. If we were to assume that this article represented their

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best work in 1949, then we would be ten years ahead of them. But I find it difficult to believe that this is their best. Our department believes very strongly that this is a morale booster and propaganda effort by some second-rate Russian meteorologist describing the former achievements of their first-rate meteorologists to the workers in the field. Meanwhile the first-rate meteorologists, the really topnotch workers in the universities, are probably already on an entirely different line of attack.

Russian meteorologists couldn't be as backward as this article indicates. They obviously can follow our developments as well as those of British meteorology and Swedish meteorology. It is inconceivable that they are not aware of the present partially successful attempts at numerical prediction now going on in the West. In other words, we, about 1947, after the initial work of Dr Jules Charney, have been developing these numerical techniques based on the vorticity equation. Nowhere in Zubyan's paper is the word "vorticity" mentioned or even hinted at. In fact, many of our important concepts in the last five or ten years are not even hinted at. We adopted an entirely different approach in 1947 which only seven years later has already led to the present establishment of the Joint Numerical Prediction Center in Washington. This very rapid development has been going on in the West for seven years. It represents a really huge step forward in meteorology. In Zubyan's article there is no suggestion that such a thing is going on in the USSR; but we feel strongly that they have to be aware of this new trend.

6. On the positive side, this article shows a knowledge of the basic equations we use and considerable mathematical ability; and it also shows, although Zubyan usually refers to the West in a derogatory manner, that in 1949 they were keeping somewhat abreast of certain developments in Western meteorology. For instance, the wave theory of cyclones is mentioned, and some reference is made to the work of Ertel, a topnotch German meteorologist. They evidently do keep in contact with what is going on in the outer world.
7. The technical basis of Zubyan's article, assuming that it is the best they could do in 1949, is quite shaky. I doubt that it is theoretically sound. I was prejudiced from the start by Zubyan's method of computing the advective pressure change, that is the first approximation pressure change, from the first law of thermodynamics. Since it is impossible to evaluate all the terms in that law, Zubyan has to neglect certain of these terms. He retains only the local temperature change, the horizontal temperature advection, and the local pressure change. Basically he is computing the local pressure change from the difference between the horizontal temperature advection and the local temperature change. In our own use of the first law of thermodynamics, we know very definitely from measurements that the term involving the pressure change is of an order of magnitude smaller than the other terms and that in any problem to which we apply this equation it is customary to neglect this pressure tendency term; in other words, it is considered equal to zero. And yet Zubyan uses that very term to compute the pressure change. He gets away with this by computing the local temperature change from essentially the advection of the surface temperature field by the winds at the tropopause. It is impossible to say how he arrives at this result because so many steps are missing at that point. He says simply that Kibel arrives at the final equation through a series of ingenious assumptions. I think they must be extremely ingenious. But the effect of all the manipulation is simply this - that he reduces the local temperature change in such a way that the difference between the advective temperature change and the local temperature change is enlarged and therefore raises the order of magnitude of the pressure change by one and makes it of the same order of magnitude as those observed in the atmosphere. And in this manipulation it turns out that warm advection will be associated with pressure fall and vice versa, which we know empirically to be more or less true in the atmosphere. It is somewhat by accident that the thing works at all. I might just add that in Sverre Petterssen's well known textbook, in the very beginning, he writes down the first law of thermodynamics in order to show how the local temperature change may be computed and then just throws out this term which is all-important to the Soviets. This leads me to believe that the theoretical development in this article is not sound. In fact I could go a step further. In the second approximation, they compute the geostrophic deviations from the acceleration of geostrophic wind in a fashion which we have tried here and found to be

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wanting; in fact we got ridiculous results. Again, it is hard to believe that this second approximation can give anything worth while in view of this assumption.

8. The question of how good is the advective-dynamic theory has already been answered by Professor Haurwitz in the American Meteorological Society Bulletin. He showed that in the testing done at MIT the theory proved faulty in practice.
9. How far can the Russians go with this method before starting on a fresh line of attack? The only possible further step for Kibel to make would be to build up successively higher approximations; and it is evident that he did this between 1940 and 1946. However, these higher approximations involve higher derivatives which cannot be measured from meteorological data. In other words, they are only a theoretical approximation. They cannot be computed. Then in view of all this I believe that with the second approximation the theory for all practical purposes has reached the limit of its development, and that therefore qualitative results derived from this equation have also advanced as far as they are likely to be advanced; which means that if the Soviets are going to progress further they will have to embark on an entirely new line of attack. Our present line of attack in the US is numerical forecasting based on various forms of the vorticity equation. We too are handicapped by the same difficulties the meteorologists in any country would be - namely, that you cannot take the meteorological equations in their raw form and make computations from them. They have to be manipulated in certain ways and fitted into certain approximations. We believe that the success of our methods obtained up to now proves that we have a fairly realistic model of the atmosphere in these equations.
10. The primary impetus to numerical prediction has been the high-speed computer. I have no knowledge of computer development in the USSR. We in our department, however, feel strongly that there is no reason to assume that our skill as forecasters in this country, Great Britain, or any other country in western Europe, is superior to that of the Russians; and that even with numerical methods, there is no reason to think that in the very near future our forecasting skill will be any better. Right now forecasting is still somewhat of an art. We have various guiding physical principles which definitely help us in our predictions; but basically forecasting is a matter of extrapolation of current weather conditions and trends, which largely depends upon a man's experience and know-how. I don't believe that the Russians use Kibel's method quantitatively now. They couldn't. There wouldn't be any value in it. They must know by now it is worthless and probably knew it even in 1949 when Zubyan's article was written. Again, however, a qualification is necessary. In order to use Kibel's method qualitatively, the Russians prepare certain basic charts, which we also consider important. They evidently use the surface weather map as their primary map, just as we do, and their second most important chart is a 700 millibar chart on which they superimpose the thickness lines from 1000 millibars to 500 millibars. In using Kibel's method qualitatively they observe certain relationships between the pressure and temperature configurations which could be of value in forecasting even though there is no theory back of the use of them which is really sound. It is possible that a mixed theoretical and empirical approach could for all practical purposes give them about the same forecast as we get. We really do the same thing here. We throw in everything except the kitchen sink and sometimes it seems that we have to forecast pretty much from intuition or cumulative experience and then try to justify our forecast by various things appearing on the maps. Maybe that's what the Russians do. They probably are able to make reasonably good forecasts and then they try to use some of Kibel's theories to say that their forecasts make sense.
11. I feel quite strongly that this article of Zubyan's is a secondary, popularized piece of work for the consumption of laymen. The shift in attention to vorticity first started with Rossby toward the mid-1940's and then with Charney's work in the late 1940's. It is possible that in 1949 the Soviets were just becoming acquainted with Charney's work; but it certainly could not have taken them very long, once becoming aware of it, to understand it and actually to go ahead and try to develop it themselves. The advanced Soviet theorists would have been working on vorticity in 1949. This Zubyan might simply be one of the top men in the Russian Weather Service. In other words, he would be a person of university training, well grounded in the fundamental precepts of meteorology, able to express himself well and able

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to understand the works of the investigators and topnotch research men in universities. In other words he probably would be sort of a go-between. If he were a first-rate research man he probably would be engaging in original research and writing for Russian meteorological journals rather than writing articles of this sort. The statement of the publishers: "To popularize the recent developments of Soviet scientists" - is the key to the whole thing. We do the same thing in the US. Some of the material written for our own weather service is also relatively weak in meteorological background in many cases. It would be very difficult, for instance, to write a brochure on numerical predictions which would be very meaningful. We would simply talk in a very popular manner on what we hope to do with it but would never actually go into the theory of numerical predictions for the man in the field. I think it would be very, very dangerous for us to try to judge the status of Soviet meteorology in 1949 from this article.

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